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Too Young to Leave the Nest? The Effects of School Starting Age

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The Effects of School Starting Age*

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Abstract

Does it matter when a child starts school? While the popular press seems to suggest it does, there is limited evidence of a long-run effect of school starting age on student outcomes. This paper uses data on the population of Norway to examine the role of school starting age on longer-run outcomes such as IQ scores at age 18, educational attainment, teenage pregnancy, and earnings. Unlike much of the recent literature, we are able to separate school starting age from test age effects using scores from IQ tests taken outside of school, at the time of military enrolment, and measured when students are around age 18. Importantly, there is variation in the mapping between year and month of birth and the year the test is taken, allowing us to distinguish the effects of school starting age from pure age effects. We find evidence for a small positive effect of starting school younger on IQ scores measured at age 18. In contrast, we find evidence of much larger positive effects of age at test, and these results are very robust. We also find that starting school younger has a significant positive effect on the probability of teenage pregnancy, but has little effect on educational attainment of boys or girls. There appears to be a short-run positive effect on earnings of beginning school at a younger age; however, this effect has essentially disappeared by age 30. This pattern is consistent with the idea that starting school later reduces potential labor market experience at a given age for a given level of education; however, this becomes less important as individuals age.

Does it matter at what age a child starts school? Older children do better on tests, but is this because they are older and, in fact, unrelated to the age they started school? Despite the dearth of convincing evidence, the popular press seems to suggest that there are benefits to "redshirting" (holding back) children in kindergarten (See NY Times, June 3, 2007). But is this the case? Are the short run benefits in terms of better performance just that: short run? And are there costs associated with finishing school and starting work later?

Much research has shown a consistent pattern that children who start school later tend to score higher on in-school tests, even after accounting for the endogeneity of school starting age.[1] However, a key limitation in the interpretation of these correlations is the inability to distinguish between the effect of school starting age and a direct age-at-test effect, as they are perfectly collinear. As a result, it could be that children who start school when they are older do better simply because they are older when they take the tests and being older provides an advantage, or it could be because there are direct benefits to starting school at an older age.

Using data on the population of Norway, we are able to separate these two effects using IQ test scores measured outside of school, at the time of military enrolment when students are around age 18. The rule in Norway that children must start school the year they turn 7 provides a discontinuity in school starting age for children born around January 1st and provides an instrument for actual school starting age. Importantly, there is also variation in the mapping between year and month of birth and the year the test is

[1] This includes a cross-country study by Bedard and Dhuey (2006) and country-level studies by Fredrikson and Ockert (2006) for Sweden, Puhani and Weber (2007) for Germany, Strom (2004) for Norway, Crawford, Dearden and Meghir (2007) for England, McEwan and Shapiro (2008) for Chile, and Elder and Lubotsky (2007) for the US.

taken, allowing us to distinguish the effects of school starting age from pure age effects. Cognitive scores around age 18 are particularly interesting as it is about the time of entry to the labor market or to higher education and so these scores are more relevant to the labor market than scores in kindergarten or elementary school.

Additionally, we study the effects of school starting age on longer term outcomes including educational attainment, early fertility, and adult earnings. While this is methodologically less complicated than studying in-school tests because age of measurement and school starting age are not perfectly collinear, the literature has been hindered by a paucity of data. Given the complications created by school leaving age rules in the US, European data are attractive when studying education and earnings.[2] Educational attainment has been studied in the literature and it has generally been found that older starters average modestly higher completed education.[3] However, ultimately adult earnings are a very important outcome and we add to the literature by being the first study to track cohorts of men and women from ages 24 to 35 and analyze how the impacts of school starting age change with age.

There has been some more recent evidence that the timing of births may be manipulated by parents based on school starting age cutoffs; to the extent that this is true and that these parents may be "different" on other dimensions as well, estimates in the literature may be biased. Unlike the previous literature, we can control for sibling fixed effects to take account of this possibility.

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[2] Studies using U.S. data have suffered from the fact that compulsory schooling laws specify minimum school leaving ages rather than grades so early starters have completed more education at the minimum dropout age. Therefore, historically, persons whose quarter of birth predicts starting early have on average higher schooling and higher earnings (Angrist and Krueger 1991). Dobkin and Ferreira (2007) finds younger starters also obtain slightly higher education in more recent U.S. cohorts.

[3] See Puhani and Weber (2007) and Fertig and Kluve (2005) for Germany, and Fredrikson and Ockert (2006) for Sweden.

There are a number of reasons why school starting age may have long-run effects on children's outcomes, although the sign of this effect is theoretically ambiguous. The potential advantages of an early starting age could include the following: Starting school younger means finishing school at a younger age, which implies more time for the individual to earn returns on their investment in human capital. Starting school younger may also be advantageous to the extent that children learn more at school than at home (or be a disadvantage if the opposite holds true), which could affect their long run trajectory. Finally, parental investment in their children may also depend on school starting age – parents may provide more help to children who are young for their grade level.

There are also potential disadvantages of an early starting age (and some of these could work in the opposite direction as well). It is possible that children cannot learn as well in school earlier in their developmental life. In addition, social development may depend on a child's age relative to that of his/her classmates; if being relatively older is advantageous, then it might be better to start children later (and vice versa). To the extent that older children have an advantage on exams in school (by mere virtue of the fact that they are older when they take the test and hence know more), they may do better in the long run. Given these potentially offsetting effects, the true causal relationship becomes an empirical question.

We find evidence for a small positive effect of starting school younger on IQ scores measured at age 18. In contrast, we find evidence of much larger positive effects of age at test, and these results are very robust. When we examine other outcomes, we find that school starting age has a significant effect on teenage pregnancy among girls but no strong effect on education among girls or boys. We also find that there appears to be a short-run positive effect on earnings of beginning school at a younger age; however, this effect has essentially disappeared by age 30. This pattern is consistent with the idea that starting school later reduces potential labor market experience at a given age for a given level of education; however, this becomes less important as individuals age.

The paper unfolds as follows. Section 2 presents the relevant literature. Section 3 describes our methodology and contrasts it to other approaches in the literature. Section 4 discusses relevant institutional details in Norway, and Section 5 provides a data description. Section 6 presents our results and Section 7 concludes.

2. Relevant Literature

Despite the importance of the distinction, there is little solid evidence as to the role of school starting age (SSA) versus test age (AGE) in determining school test scores. While children are in school, researchers are faced with the identity that

AGE AT TEST = SCHOOL STARTING AGE + YEARS OF SCHOOLING

Most of the literature has compared test scores of children who are in the same grade and so has in fact estimated the combined effects of SSA and AGE.

Given the difficulty with separating out the two effects, a number of recent papers try to infer the role of age versus school starting age by looking either at early test scores or at changes in scores over time.[4] Elder and Lubotsky (2007) show that there are strong age effects in the fall of kindergarten year, before children could have been much affected by formal schooling. Elder and Lubotsky (2007) and Cascio and Schanzenbach

[4] For example, Datar (2006) finds that achievement changes between kindergarten and first grade are not highly correlated with age at school entry.

(2007) also show that effects of age-at-school-entry on test scores tend to get smaller as children move to higher grades. Together these papers imply that the estimated starting age effects partly reflect the endowment differences between students when school starts and that there is little evidence that students learn more in school if they are older when they start. However, none of this work is able to directly disentangle the direct effect of age at test from that of school starting age.

While most of the literature controls for time in school, there is another series of papers that controls for age at test. There is some evidence that, when tested at the same age, young children score better on in-school tests if they started school younger and hence have spent more time in school. Cahan and Cohen (1989) study Israeli elementary school children and Elder and Lubotsky (2007) compare U.S. children around age 6 but with different predicted starting ages (based on month of birth). However, the bulk of this evidence is for very young children in kindergarten and elementary school and it is not clear that these findings generalize to older ages relevant to the labor market

Most similar methodologically to this paper are papers by Crawford, Dearden and Meghir (2007) and Cascio and Lewis (2006), both of which rely on multiple sources of variation to identify the effect of school starting age on children's test scores. Crawford, Dearden and Meghir (2007) use the fact that there is variation in school starting age across local education authorities (LEAs) in Britain to separately identify the effect of school starting age from age at test effects on in-school tests. While some LEAs have only one entry point (with one cutoff date), other LEAs have two entry points (with some children starting in September and some starting in January) or even three entry points (with children starting school in September, January, and April). So while the school start cutoff in Britain is September 1st, August-born children start school in September in some LEAs and later in the year in others. Thus, the effect of SSA can be distinguished from that of test age by comparing August and September born children who are in LEAs that have different policies. They find that age at test is the biggest factor; however, a limitation of this methodology is that the different school starting policies may themselves be disruptive or lead to changes in curriculum and so may impact both August and September born children.[5]

Cascio and Lewis (2006) examined the role of schooling on student performance on the Armed Forces Qualifying Test (AFQT) in the NLSY. A nice feature is that it focuses on older children and there is variation in school cutoff ages (arising from across state variation) as well as variation in the age at which individuals take the test. This suggests that the authors are able to identify the effect of school starting age while controlling for age (they actually interpret these estimates as effects of schooling but as described above, schooling and school starting age are perfectly collinear for in-school children, conditional on age). Unfortunately, likely due to a relatively small sample, the authors find very imprecise statistically insignificant effects of school starting age when controlling for age. Because we have data on the population of Norway, we are much better able to identify the effect of school starting age controlling for age.[6]

There is also a recent literature examining the relationship between school starting age and longer run outcomes such as fertility, educational attainment, and earnings.[7]

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[5] Another limitation of this paper is that the authors know the LEA a child is in at the time of the exam, not when they started school, suggesting that results for the earliest tests scores may be most valid.

[6] Mayer and Knutson (1999) also find some evidence that quarter of birth matters for test scores in the CNLSY.

[7] Bedard and Dhuey (2007) use variation in school starting age within states over time in the United States to identify the effect of cohort age and absolute age (netting out relative age effects). Their findings suggest a significant positive effect of increasing the school starting age on wages.

While methodologically this is less complex because there is no link between date of measurement and time in school, the literature has been limited by the absence of good data. For example, the most thorough previous study of earnings by Fredrikson and Ockert (2006) has only one year of earnings data and so cannot distinguish between cohort and age effects. We are the first study to track cohorts of men and women from ages 24 to 35 and analyze how the impacts of school starting age change with age.[8]

3. Methodology

Identification Strategy

We first describe the empirical strategy we use when our outcome variable is completed years of education, log earnings, or probability of having a teenage birth. We then describe the adjustments we make when we look at IQ as an outcome; as described earlier, when we look at IQ we need to account for the fact that we control for age at test.

Education, Log Earnings, and Teenage Fertility Outcomes

Our equation of interest is as follows:

$$Y_i = \beta_0 + \beta_1 SSA_i + X_i'\lambda + \varepsilon_i \qquad (1)$$

where Y is the outcome under study, SSA is the school starting age, and X is a vector of controls that includes year of birth indicators and a local linear trend. Because the school cutoff is at the beginning of the year, we redefine year of birth to run from July to June rather than from January to December (so the discontinuity is now at the middle of our re-defined "year").[9] We also include a local linear trend that is centered at the

[8] Work by Skirbekk, Kohler, and Prskawetz (2004) use Swedish data to examine the role of birth month on the timing of births and marriages; while they find a significant effect on average fertility, we are the first study we know of to examine early fertility.
[9] Fredrikson and Ockert (2006) also use this redefined-year approach in their Swedish study.

discontinuity (a trend ranging from 1-12 and centered at December/January). Together the year of birth indicators and local linear trend allow for cohort effects such as secular increases in educational attainment over time.

Because parents may be able to manipulate school starting age, we need to find an instrument to identify the true relationship between school starting age and outcomes. Our exogenous variation in school starting age comes from variation in month of birth and the administrative school starting rule in Norway – children born in December start school a year earlier than children born in January, with a December 31 cutoff. Therefore, we estimate equation (1) by 2SLS using the expected school starting age (ESSA) as an instrument for the actual school starting age. In Norway during our sample period, the legal rule was that children must start school in the year they turn 7. We measure the ESSA as equal to 7.7 – (month of birth -1)/12. This takes account of the fact that school starts in August and the cutoff date is at the beginning of the year. Given the ESSA is determined only by month of birth and not by parental choice, it seems reasonable to treat it as exogenous and use it as an instrument for the actual SSA.

For ESSA to be a valid instrument for SSA, two conditions must be satisfied. First, it must be random which children are born in different months of the year; this could be violated if different types of families have children at different times of the year. We attempt to address this issue in a number of ways. As a robustness check, we include family characteristics in our regression and show our resulting estimates are very close to those estimates without these controls. In addition, and perhaps more convincing, we are also able include family fixed effects as a check on this possibility.

Second, it must be that there is no direct effect of being born at a particular time of the year on child outcomes. While there is some evidence of small differences in health outcomes across season of birth (Bound and Jaeger 2000), the balance of previous evidence is that these differences are not nearly large enough to make much difference. Importantly, our critical comparison is between December- and January- born children, so differences between summer and winter born children are largely irrelevant.

It is important to note that the thought experiment here is that we vary one individual's school starting age while holding that of everybody else fixed. Thus, we are essentially changing two things: the age the individual starts school and the relative age of that child in the classroom (from relatively younger to relatively older). Given that we have no information on variation in the ages of other children in the class, we cannot tell whether our estimated effect of starting a year later matters because it makes the child a year older in absolute terms or because it makes the child older relative to his classmates.

IQ Scores as Outcomes

As described previously, when we study IQ scores at age 18, we add a control for the age of the person at the time of the test (AGE):

$$IQ_i = \beta_0 + \beta_1 SSA_i + \beta_2 AGE_i + X_i{}'\chi + \varepsilon_i \qquad (2)$$

In Norway, there is a relationship between month of birth and when persons are called to do the test. As an example, in some years, individuals who were born in January and February are called to take the exam in one year while individuals born after February (in the same year) are called to take the exam a year later. Since in most years, the birth month cutoff for the test is not December and so is *not* perfectly correlated with expected school starting age, we are able to disentangle school starting age effects from general age effects.

A complication that arises is that not all men take the test in the year in which they are supposed to do so. This type of deviation can occur due to illness, absence abroad etc. As a result, age at the time of the exam is potentially endogenous. Conceptually similar to the case of school starting age, we use the year in which you were supposed to take the test as an instrument for the age at which did take the test. To do so, we define test feeder groups for each test year based on year and month of birth. For example, all persons born in calendar year 1951 were supposed to take the test in 1970, so they are all members of the 1970 feeder group. On the other hand, persons born between April 1961 and April 1962 were supposed to take the test in 1980, so they constitute the 1980 feeder group. We instrument for AGE using the indicator variables for the feeder group to which each individual belongs. This exploits the discontinuity that while cohort of birth changes smoothly, persons born in April 1961 are almost a year older taking the test than persons born in March 1961. Given that over 90% of men do the test in the year they are supposed to, the first stage relationships are extremely strong and there is no concern about weak instruments.

Appendix Figure 1 illustrates the discontinuity between test month cutoff and age at test in our data. In the figure, zero corresponds to cutoff birth months and -1 to birth months that are one month previous to a cutoff birth month etc. The figure shows how average age at test varies depending on where the person's birth month is relative to the relevant cutoff birth month for that individual. The very large discontinuity in age at test at the cutoff birth months is very clear.

As described earlier, conditional on age, school starting age is typically perfectly correlated with time-in-school when the outcome is measured while still in school. In Norway, many boys take the military IQ tests while still in school; our estimates for IQ will therefore provide an upper bound on the benefits of starting school young, holding schooling constant. Later we evaluate the role played by time-in-school by providing separate estimates for persons who had finished schooling by the time of the test.

Additional Specifications

In addition to the 2SLS procedure described above, we also estimate two additional specifications for all our outcomes:

Discontinuity Sample

The specification in equations (1) and (2) uses all months for identification of the SSA effect but allows other factors to impact IQ scores smoothly (linearly) through the discontinuity point.[10] As a robustness check, we also estimate our equation on the subsample of individuals born in either December or January, thereby using only the individuals born close to the discontinuity for identification. In this case, the local linear trend is unidentified and so is excluded from the estimating equation. The assumption underlying use of the discontinuity sample is that December and January observations are exchangeable so that, on average, their outcomes differ only because of the difference in their school starting ages.

Family Fixed Effects

There has been some recent evidence that the timing of births may be manipulated by parents based on school starting age cutoffs; to the extent that this is true and that

[10] Note that, even using all months, the discontinuity in ESSA is necessary for identification as, in the absence of the jump in January, ESSA would be perfectly correlated with the linear trend.

these parents may be “different” on other dimensions as well, our estimates may be biased.[11] However, because we have data on the population of Norway, we can also investigate the relationship between school starting age and long run outcomes *within* families, thereby differencing out any time-invariant family qualities. To do so, in some specifications we estimate the 2SLS regressions with additional dummy variables for each set of siblings. These specifications will provide consistent estimates unless the timing of births amongst siblings is correlated with the counterfactual outcomes of the children. This seems unlikely as child endowments are not known before birth but could arise if, for example, parents decide to strategically time the second child in response to indications that the first child was low quality.

4. The Norwegian Childcare and School System

In Norway, children under the age of seven can be placed in a daycare facility; Norway has both public and private facilities. However, prior to the mid 1970s, labor market participation rates for married women were relatively low, with rates in the 35% range in the 1960s and in the 40% range in the early 1970s. In addition, families faced a shortage of daycare facilities during that period. As a result, prior to 1980, daycare enrollment for children between the ages of 3-6 was around 10 percent or less, with a large increase during the 1980s.[12]

In the late 1970s through the 1980s, there was a large increase in labor force participation rates of married women, to over 70 percent by 1990. This was accompanied by a larger increase in preschool enrollment. The expansion that began in the 1980s

[11] See Crawford, Deardon, and Meghir (2007).

[12] Up to 1980, most daycare facilities were located in urban areas and most catered to the children of working mothers.

represented a particularly sharp increase in coverage in rural areas. Appendix Table 1 shows preschool coverage by age of child from 1963-2002. (Source: Pettersen, 2003). While our data broadly cover children aged 6 between 1968 and 1994, most of our outcomes rely on children born in the earlier part of the period. This suggests that, during the time period relevant to our sample, most children were at home prior to enrollment in school, either with their mother or an informal childcare provider such as a grandparent or a neighbor.

In terms of schooling, all compulsory education in Norway is free. Since 1997, schooling has been compulsory from age 6 to 16 (10^{th} grade). However, the cohorts we consider faced a school starting age of 7 and 9 years of compulsory schooling (until age 16). Schools are generally run by the local Municipality and there is no streaming by ability during the years of compulsory schooling.[13]

Norway has mandatory military service of between 12 and 15 months (fifteen in the Navy and twelve in the Army and Air Force) for men between the ages of 18.5 (17 with parental consent) and 44 (55 in case of war). However, the actual draft time varies between six months and a year, with the rest being made up by short annual exercises. Students have tended to attend university after completing military service with the result that average age of college students is 22 in Norway (Mortimore et al, 2004).

5. Data

Our primary data source is the Norwegian Registry Data, a linked administrative dataset that covers the population of Norwegians up to 2006 and is a collection of different administrative registers such as the education register, family register, and the

[13] There are very few private schools in Norway and only about 2% of all pupils attend them.

tax and earnings register. These data are maintained by Statistics Norway and provide information about educational attainment, labor market status, earnings, and a set of demographic variables (age, gender) as well as information on families.[14] To ensure that all individuals studied went through the Norwegian educational system, we include only individuals born in Norway. We have information on school starting age for cohorts born from 1962 onwards and our analysis focuses on the 1962-88 cohorts.

The IQ data are taken from the Norwegian military records from 1980 to 2005. In Norway, military service is compulsory for every able young man. Before entering the service, their medical and psychological suitability is assessed; this occurs for the great majority between their eighteenth and twentieth birthday. IQ at these ages is particularly interesting as it is about the time of entry to the labor market or to higher education.

The IQ measure is a composite score from three speeded IQ tests -- arithmetic, word similarities, and figures (see Sundet et al. [2004, 2005] and Thrane [1977] for details). The arithmetic test is quite similar to the arithmetic test in the Wechsler Adult Intelligence Scale (WAIS) [Sundet et al. 2005; Cronbach 1964], the word test is similar to the vocabulary test in WAIS, and the figures test is similar to the Raven Progressive Matrix test [Cronbach 1964]. The composite IQ test score is an unweighted mean of the three subtests. The IQ score is reported in stanine (Standard Nine) units, a method of standardizing raw scores into a nine point standard scale that has a discrete approximation to a normal distribution, a mean of 5, and a standard deviation of 2.[15] We have IQ scores on about 84% of the relevant population of men in Norway.[16] [17]

[14] See Møen, Salvanes and Sørensen [2004] for a description of these data.

[15] The correlation between this IQ measure and the WAIS IQ has been found to be .73 (Sundet et al., 1988).

[16] Eide et al (2005) examine patterns of missing IQ data for the men in the 1967-1987 cohorts. Of those, 1.2 percent died before 1 year and 0.9 percent died between 1 year of age and registering with the military

In terms of educational attainment, we measure education at the oldest age possible for each individual i.e. in 2006.[18] To get as close as possible to completed education, we do not include anyone in the education sample who is aged less than 27 in 2006.

In terms of teenage childbearing, we study two related outcome variables. The first is whether a woman has a child as a teenager, and the second is whether a woman has a child within 12 years of her expected school starting age. While the former is the more standard measure of teenage childbearing, the latter is plausibly a better measure of whether early motherhood is likely to disrupt human capital accumulation and hence later earnings potential. Given that most of our sample completes at least 12 years of schooling and 12 is the modal level of schooling, this outcome variable measures whether women are likely to find it difficult to obtain the normal level of education because they have children.

We construct the first variable by restricting the sample to women aged at least 36 in 2006 and denoting a teen birth if they have a child that is aged at least 16 in 2006 who was born before the woman was aged 20.[19] The second dependent variable is constructed

at about age 18. About 1 percent of the sample of eligible men had emigrated before age 18, and 1.4 percent of the men were exempted because they were permanently disabled. An additional 6.2 percent are missing for a variety of reasons including foreign citizenship and missing observations. There are also some missing IQ scores for individuals who showed up to the military.

[17] One concern is that missing IQ is nonrandom and is related to SSA. To examine this, we regressed an indicator whether IQ is missing on SSA using the standard specification; while the OLS results are positive and significant, 2SLS estimates were small and insignificant. We got similar results when we examined missing earnings.

[18] Our measure of child educational attainment is reported by the educational establishment directly to Statistics Norway, thereby minimizing any measurement error due to misreporting. This educational register started in 1970. See Møen, Salvanes and Sørensen [2004] for a description of these data.

[19] This sample restriction is required because to know whether a woman had a teen birth we need to see whether or not a child appears in the panel with whom she has a less than 20 years age gap. The effect is that the cohorts we use are born between 1963 and 1969.

using the same sample. On average in our sample, 8% of women have a birth as a teenager and 6% have a birth within 12 years of the expected school start date.

Finally, earnings are measured as total pension-qualifying earnings reported in the tax registry and are available from 1986 to 2005. These are not topcoded and include labor earnings, taxable sick benefits, unemployment benefits, parental leave payments, and pensions.

For the purposes of studying earnings and employment, we restrict attention to individuals aged between 24 and 35. In this group, about 94% of both men and women have positive earnings. Given this high level of participation, our first outcome is log(earnings) conditional on having non-zero earnings. Since the results for this variable encompass effects on both wage rates and hours worked (and since the earnings measure picks up earnings from summer work by students and other short-term activity), we also study the earnings of individuals who have a strong attachment to the labor market and work full-time (defined as 30+ hours per week). To identify this group, we use the fact that our dataset identifies individuals who are employed and working full time at one particular point in the year (in the 2nd quarter in the years 86-95, and in the 4th quarter thereafter).[20] We label these individuals as full-time workers and estimate the earnings regressions separately for this group. About 52% of our male sample are employed full time aged 24 but this increases to 78% by age 35. The equivalent figures for women are 42% and 50%.

Table 1 presents summary statistics for our sample.

[20] An individual is labeled as employed if currently working with a firm, on temporary layoff, on up to two weeks of sickness absence, or on maternity leave.

6. Results

First Stage Estimates

In Norway during our sample period, the legal rule was that children must start school in the year they turn 7. In practice, compliance with this rule was almost perfect for the cohorts we study. (Appendix Table 2 shows compliance rates by birth year for cohorts born 1962-1988.)[21] This is not surprising as parents had to formally apply for an exception from the rule and the application had to be approved by health and school specialists as well as by the local government (Strom 2004). The high compliance rates are reassuring as they imply that our IV estimates can be interpreted as an approximation to the average treatment effect of school starting age rather than the usual LATE interpretation.[22]

We report first stage estimates by gender in Table 2 for the full sample and for the discontinuity sample. Because the first stage estimates are quite similar across the particular samples used for different outcomes, for parsimony we have chosen to report first stages for the full set of cohorts born between 1962 and 1988. Using the full sample, the first stage coefficient on ESSA is .80 for men and .82 for women. These change very little when family fixed effects are added to the specification in column (3).

As one might expect, compliance rates are lower for children born in December and January than for persons born during the middle of the year (see Appendix Table 3). This can be seen in the lower first stage estimate for ESSA when the discontinuity sample

[21] Bedard and Dhuey (2006), using TIMMS data, show that in 4th grade only 2% of Norwegians are not in the predicted grade given their month of birth and Strom (2004) reports that 99.5% of the 1984 cohort of Norwegian public school students in the 2000 PISA are in grade 10 (as they should be).

[22] Consistent with recent popular press, we find that it is the better educated mothers who are more likely to be noncompliers; however, counter to this anecdotal evidence on "redshirting", these mothers are actually more likely to start their children *early*. ("When Should a Kid Start Kindergarten?" New York Times, June 3, 2007)

is used (see column (2) of Table 2). However, even in this sample, the first stage estimates are about .75.

IQ Results

Our results for IQ test scores are presented in Table 3 (we have this information only for men). We first present the OLS results (Column 1), which suggest that SSA has a large negative effect on military test scores. The coefficient of -.8 implies that going to school one year later reduces test scores by 4/5 of a stanine or almost half a standard deviation. In contrast, the OLS estimates suggest no impact of age at test, which runs counter to our prior that older boys score higher on tests. Of course, the OLS estimates may be suffering from selection bias, with less-able children having their school entry, and possibly their test-taking, delayed.

To address this issue directly, from this point forward we treat SSA and age at test as endogenous and use the 2SLS strategy described previously. In contrast to the OLS results, the 2SLS estimates show a strong positive effect of age at test on IQ. The estimate implies that being one year older when taking the test increases the score by about .22; this is one fifth of a stanine and about one tenth of a standard deviation. Additionally, the effect of SSA is still negative and statistically significant but is much smaller, suggesting that starting school a year later reduces IQ scores by about .06, about one twentieth of a stanine.

Taken together, the age-at-test and SSA coefficients provide a prediction of what one would obtain if a boy started school a year later and, as a result of taking the exam with his school entry cohort, took the exam a year later. In this case, the estimated SSA effect would be the sum of the true SSA effect and the age-at-test effect. This equals .16, which is about 8% of a standard deviation. This is somewhat lower than the findings in the literature that use in-school tests (for example, using 9th grade GPA in Sweden, Fredriksson and Ockert find a positive effect of SSA that is about 20% of a standard deviation). The smaller effect is unsurprising given that our test-takers are older and that the IQ tests probably measure fixed components of intelligence to a greater extent than in-school tests.

Robustness checks

In Columns (3) and (4) in Table 2, we investigate the robustness of our 2SLS estimates to alternative specifications. In Column 3, we focus specifically on individuals who are born in the months of December or January and hence are right around our point of discontinuity. By restricting the sample, it is clear where our identification is coming from. As expected, the sample size is greatly reduced and, though consistent with our earlier findings, the results are less statistically precise.

Finally, the fourth column includes family fixed effects estimates and controls for the birth order of the child. The number of observations is lower for these specifications because we exclude families in which there are not at least two boys with IQ scores. Again, the results are quite robust to using even within-family differences for identification. Apparently, there are no serious biases arising because of strategic birth timing by parents.

In Appendix Table 4 we report a set of alternative specifications to reassure that our findings are robust to specification. These include allowing the local linear trend to be different for each birth year, including a quadratic local trend, allowing the local linear trend to change slope after January, including a quartic in cohort defined at the monthly level, and including controls for maternal education, birth order, and family size. None of these specifications provide appreciably different estimates and so we will focus on the 4 specifications in Table 3 for the remaining outcomes.[23]

Is the SSA effect a time-in-school effect?

While the test is not administered in school (and is, in fact, unrelated to schooling), there are many individuals in our sample who have not finished schooling at the time of the test. In this case, the estimated school starting age effect will encompass the fact that later starters have spent less time in school (since, for example, among individuals who ultimately complete college, those who started a year later will have not only a later school starting age but one year less of education at the time of the test.)[24] To test the sensitivity of our results to this, we break our sample into those who, ex post, actually were finished with their schooling at the time of the test (i.e. those who have ten or fewer years of education in 2006) and those who have not completed their education at the time of the test (i.e. those who have at least twelve years of education in 2006).[25]

One potential problem with this approach is that completed education may be endogenous because SSA influences educational attainment. However, as we will see in

[23] While we do not report them, we have carried out similar specification checks for the other outcomes and found those estimates to be similarly robust to specification.
[24] Leuven et al. (2006) find little evidence that time in school matters for Dutch Kindergarten children.
[25] Among those who ultimately completed 12 years of education, we are observing a mix of those who did and did not complete their education at the time of the test – in almost all cases, persons with 12 years of education who are born in January and so start school late had not finished schooling at the time of the test.

the next section, there is no evidence in our data that educational attainment of men is affected by SSA.

These results are presented in Columns 5 and 6 in Table 3. When we restrict the sample to cases where both early and late starters are finished education by the time of the test, we get no statistically significant effect of school starting age and a slightly smaller (but still statistically significant) effect of age at test. Given that we have found relatively small effects of SSA on IQ in earlier specifications, this is consistent with even these small effects being largely explained by the fact that early-starters have more schooling at the time of the test (given that effect goes away in the sample where those with an earlier starting age have no education advantage). The estimates also suggest that a small proportion of the estimated age effect is actually a time-in-school effect.[26]

Other Outcomes

We are also able to examine the role of school starting age on a number of other longer-run outcomes. These include education, earnings, and teenage childbearing.

Education

There are a number of mechanisms through which school starting age can affect educational attainment. Costs and benefits of schooling may vary with SSA as they are influenced by how effectively skills are being learned in school. For example, to the extent that older children may do better in school, this could have positive spillover effects onto educational attainment. On the other hand, because later starters will be older

[26] Note that we are attributing the difference in the estimated effect of SSA to time in school and not to heterogeneous treatment effects by educational attainment.

at any educational level, their time horizon to recoup educational investments will be shorter and this will tend to depress demand for education.

In the returns to education literature, Angrist and Krueger (1991) first used quarter of birth (as a proxy for school starting age) as an instrument for educational attainment acting through compulsory schooling legislation. (Students had to remain in school until a certain age; those who started school younger would have more schooling at the time of dropping out.) However, in the Norwegian case, compulsory schooling is based on years of school completed and not age, so this is not relevant.

Table 4 presents the education results separately for men and women. As with IQ, Column 1 has OLS results, Column 2 has 2SLS estimates using the full sample, Column 3 has 2SLS estimates using the December and January subsample, and Column 4 has 2SLS estimates with family fixed effects and birth order controls.

For men, the OLS estimates are strongly negative. However, as before with IQ, there is little evidence of a causal effect of school starting age on educational attainment for men, with 2SLS estimates being very small and statistically insignificant in both the full and discontinuity samples. The only significant effect of SSA for men is in the family fixed effects specification which shows a negative effect of -.06. This is not a large effect as it implies that starting a year later reduces education by one year for about one person in twenty.

The results for women are quite similar. The OLS effect of SSA is large and negative but the 2SLS specifications give small and statistically insignificant effects. The exception is that the estimate from the discontinuity sample is statistically significant at .07 (.03). While this is positive, it is still of modest size. Overall, the evidence for men and women suggests that SSA has at best very small impacts on completed years of education.[27]

Timing of First Birth

Motherhood at young ages has been associated with many long-term economic and health disadvantages such as lower education, less work experience and lower wages, welfare dependence, lower birth weights, higher rates of infant mortality, and higher rates of participation in crime (Ellwood, 1988; Jencks, 1989; Hoffman et al., 1993; Kiernan, 1997). There is an ongoing debate as to the extent that these adverse effects of teen childbearing are truly caused by having a teen birth rather than reflecting unobserved family background differences. (See Hotz, McElroy, Sanders 2005 for an example). However, as a policy matter, efforts to reduce the rate of teen childbearing are often considered as a strategy to improve the life chances of young women.

There are at least three reasons we might expect early fertility to be impacted by school starting age. First, since it is likely to be quite costly to be in school as a young mother, starting school later may be associated with a postponement of fertility. This has been called the "incarceration effect"; while women are in school, they do not have the desire/time/opportunity to have a child. Second, since education increases human capital, additional schooling may make you "smarter" and hence decide to postpone childbearing; this might imply that later starters (who have less education at any particular age) are more likely to have children at a given age.[28] Finally, it is likely that a major effect of

[27] We have also studied whether the individual has at least 12 years of schooling as the outcome variable and found very similar results.

[28] Black, Devereux, Salvanes (2008) discuss these mechanisms in the context of the effects of compulsory schooling laws on teenage fertility.

starting school young is that the child's peer group is older than it would otherwise be. Therefore, young starters may be more likely to engage early in adult-type behaviors such as drug taking and sex.[29] While we don't observe these behaviors directly, childbearing at young ages signals sexual activity.[30]

The first panel of Table 5 presents the results for the indicator whether or not a girl had a birth as a teenager (less than 20 years old) and the second panel presents the results for the indicator whether she had a birth within 12 years of her expected school starting age. The OLS estimates suggest a small positive effect of SSA on teen childbearing. However, when we use 2SLS, we find a statistically significant negative effect of school starting age on teenage pregnancy for both the full sample and the discontinuity sample, and the coefficient is about -.018 in both specifications.[31] This implies that a three month increase in school starting age reduces the probability of teenage pregnancy by approximately 0.5 (.25*.018*100) percentage points. While the family fixed effects estimate is smaller at -.008, it should be noted that it is quite imprecisely estimated, with the standard error of .008.

When we instead consider the effect on the probability of having a birth within 12 years of the expected school start, the OLS effect of SSA is .019 (.002). The 2SLS effects of SSA are also positive and even larger: about .04 - .05. A three month increase in school starting age will increase the probability of a birth within the first 12 years of school by about 1.2 (.25*.05) percentage points. The family fixed effects estimate is also

[29] This argument is similar to that of Argys et al. (2006) who suggest that higher birth order children are more likely to engage in risky behaviors at young ages because they are influenced by their older siblings. Also, Black, Devereux, and Salvanes (2005) find that higher birth order women in Norway are more likely to have births as teenagers.
[30] As noted earlier, while Skirbekk, Kohler, and Prskawetz (2004) document a relationship between birth month and fertility but only focus on mean age of child birth.
[31] We have verified that the average derivatives of the reduced forms from probit models are very similar to the linear probability estimates and have similar sized standard errors.

about this size. The main reason for this large positive effect is that, 12 years after the ESSA, January-borns are almost one year older than December-borns and age is a prime determinant of fertility. Our estimates suggest that, although starting school older does reduce teenage pregnancy, it still increases the probability that a girl will interrupt her schooling to have a baby.

Earnings

For simplicity, assume that earnings depend on (1) labor supply, and (2) the wage rate. At age 24, some Norwegians are still in full time education and performing little paid work. Thus, at these young ages, labor supply differences are particularly important. Because early starters tend to finish schooling a year earlier, this is a major reason they should have higher earnings at young ages. At older ages (late 20s on) most individuals are working so differences in wage rates are probably the dominant reason for earnings differences. Since wages depend on human capital, they depend on skills acquired up to the end of schooling, and skills developed through work experience after schooling.

Given that, empirically, there is little impact of starting age on schooling attainment or IQ scores, the biggest effect of SSA on earnings probably comes from the fact that early starters tend to have more work experience at any age. Since age-earning profiles are concave, this should imply that the effects of starting later get more positive (or less negative) as people get older. For this reason, we exploit the fact that we have panel data on earnings from 1986 to 2005 in order to examine how SSA effects change with age. To follow persons from age 24 (when some have not finished schooling) to 35 (at which point the marginal value of a year of extra labor market experience should be getting low), we use a sample born between 1962 and 1970. Notice that a crucial feature of our data is that we can follow cohorts (and even individuals) as they age and so can distinguish between cohort and age effects. In contrast, in their Swedish study, Fredriksson and Ockert (2006) have only 1 year of earnings data and so cannot make this distinction.

For maximum flexibility, we estimate separate regressions by age for all specifications. One potential problem is that earnings may be generally higher in a particular year because of, for example, favorable economic conditions. As a result, we do not want to compare earnings in one year for December-borns to earnings in a different year for January-borns. So, as before, we redefine a birth year to include people born between July 1 and the following June and measure earnings for all individuals in the redefined birth year at the same time.

As mentioned earlier, we study the earnings of all labor market participants and the earnings of full time employees in an effort to distinguish labor supply from wage effects. We present the same four specifications as before for both log earnings of all individuals with positive earnings (Table 6 for men and Table 8 for women) and log earnings of full time workers (Table 7 for men and Table 9 for women). We run each regression by age and the reported coefficients are the effect of SSA on log of earnings. The estimated SSA effect gives the effect of school starting age conditional on age so (assuming no effect of SSA on educational attainment) can be interpreted as the benefit of spending a marginal year before starting schooling rather than after finishing schooling.

The OLS estimates for men are negative, and the negative effect gets larger as men get older. This is inconsistent with the effects of SSA wearing off with experience but is probably explained by the fact that earnings at older ages provide more information about skills and late starters are negatively selected. Unsurprisingly the 2SLS estimates are very different. For men, the main finding is that higher SSA leads to lower earnings until about age 30. This is true both for all earnings and for the subsample of full-time workers. The negative effects of SSA are greater in the discontinuity sample than in the full sample but for the most part the differences between methods are not very large.[32] Quantitatively, the initial negative effects are larger (about 10% at age 24) when all earners are included than when only full-time workers are included (about 5% effect at age 24). This is consistent with much of the earnings impact coming through differential labor supply, with older school starters working fewer hours at younger ages. The estimates for women in their 20s are generally similar to those of men but are less precisely estimated.

After about age 30, the 2SLS estimates for both men and women become close to zero and are almost always statistically insignificant. Given the large sample sizes, the estimates are quite precise and we can be confident that there is no large effect of school starting age on earnings in either direction once men or women are in their mid-30s.[33]

Figure 1 presents a visual representation of the estimates (and the upper and lower confidence intervals) from the 2SLS all-male workers specification.   At age 24, the

[32] We expect the estimates using the discontinuity sample to be a little more negative at early ages because when we use that sample we do not account for the fact that December-borns are one month older than January-borns when earnings are measured.

[33] In our standard 2SLS specification, we estimate both the SSA effect and the linear trend. The linear trend gives the value of an extra month of age, conditional on SSA, and so is the return to potential experience provided there is no cohort effect conditional on the year of birth dummies. While we do not report the estimates, we have verified that by age 35, the coefficient on the linear trend also becomes negligible and statistically insignificant. This is consistent with the return to experience being close to zero by that age.

effect on male earnings is particularly large, being about 10%. However, this negative effect disappears relatively quickly and by age 32 is almost gone. Figure 2 plots the estimates for full time male workers. Figures 3 and 4 provide the analogous picture for women.[34]

The fourth rows in Tables 6-9 provide family fixed effects estimates for log earnings. The numbers of observations are lower in these rows as we have omitted cases where there are not at least two observations from the same family in a particular regression. For men the fixed effects estimates are somewhat imprecise but are broadly similar to the other 2SLS estimates. The fixed effects estimates for women are not very informative, as the standard errors become quite high.

Additional Robustness Check

In Figures 5 – 10, we plot the estimates on the month of birth dummies from regressions that simply regress the outcome variable on the month of birth of the individual and the year of birth dummies.[35] These are essentially descriptions of the reduced forms underlying our earlier 2SLS estimates. The January effect is normed to zero in all figures. These figures provide a simple description of the raw data that provides intuition for the model estimates. It is important to remember that each point in the figures is an estimate and so much of the variation can be attributed to sampling error. Even still, the reduced form effects of ESSA underlying our 2SLS estimates are clearly

[34] One might still be concerned that 35 is too young an age to cease the analysis. We have information on ESSA, but not SSA, for cohorts born between 1950 and 1962 and we have used the 1950-1965 cohorts to estimate the reduced forms all the way from ages 22 to 40. As can be seen in Appendix Figures 2 and 3, the ESSA effect between 36 and 40 is always close to zero and never statistically significant. Given the generally high compliance rates, this suggests the SSA effect is also very small for these ages.

[35] We do not have an analogous figure for IQ because, given the age-at-test effects, there is no clear relationship between month of birth coefficients and the estimated SSA coefficient.

visible for teen pregnancy and for earnings at age 24, as the jump between December and January is very apparent. On the other hand, the basis for finding no SSA effect on male education and earnings at age 35 is also obvious, as there is no jump between December and January for these outcomes.

Heterogeneous Effects of SSA

One concern might be that, by looking at the entire sample, we are missing important differences across the distribution of children. One might expect the effects of school starting age to differ based on the family's characteristics. For example, children from poorer families may be more at risk and hence suffer most from being young in school; wealthier families may be able to better offset any negative school effects. On the other hand, the advantage of school environment over home environment could be greater for children from poorer backgrounds.

To examine this directly, we regress each outcome (by gender) on a variety of family background characteristics (mother's education, family size, and birth order) and obtain a predicted value for each individual. Using this predicted value as our index of family background (essentially just a weighted average of the three family background characteristics), we divide the sample into 4 quartiles and present the results separately for the first quartile, the second and third quartiles, and the fourth quartile. Table 10 presents these results.

As can be seen, there is little evidence of heterogeneous effects when the outcome variables considered are IQ, teen pregnancy, or educational attainment. However, the effect of SSA on the probability of giving birth within 12 years of starting school is higher for less advantaged groups (note that, with the teenage pregnancy outcomes, the highest quartile is the most disadvantaged, as the outcome is a negative one).[36]

The effect of SSA on male earnings at age 35 is statistically negative for the bottom quartile but insignificantly positive for the other quartiles. This difference is statistically significant and suggests that starting school later has a negative impact on earnings of men from disadvantaged backgrounds but has no negative effect on other men. Interestingly, the estimates for earnings at age 24 show the opposite pattern (note that for this outcome we split into quartiles based on predicted earnings at age 35 because many people are not fully engaged in the labor market at age 24). At age 24, the effects of starting school later are most negative for higher quartiles. This can be rationalized by the fact that the higher quartiles disproportionally include persons who complete a lot of education and starting later is likely to cause them to be out of the labor market at age 24.

7. Conclusions

The literature has come to two broad conclusions about the relationship between school starting age and children's outcomes. First, much research has shown a consistent pattern that children who start school at older ages tend to score higher on in-school tests. The second broad conclusion in the literature is that, when tested at the same age, very young children score better on in-school tests if they started school younger and hence have spent more time in school. These findings suggest that school starting age may have significant effects on the outcomes of adults. In this paper we find that this is, for the most part, not the case. Despite the fact that the effects of SSA on in-school tests in

[36] This result is to some extent a mechanical one – the probability of giving birth within the first 12 years of school is only 0.02 in the bottom quartile and so the effect of SSA on this probability cannot be larger than .02.

Norway are as large as those in the U.S. (Bedard and Dhuey 2006), the long-run effects of SSA seem very modest. For men, there appear to be no long-term effects on education or earnings, and the effects on military test scores are very small when one allows for age-at-test effects. For women, there is little evidence of large impacts on educational attainment. An intriguing result is that starting early increases the likelihood of giving birth as a teen but reduces the likelihood of giving birth within 12 years of starting school. Overall, there is not much evidence to suggest that there are strong reasons for parents to hold their children out of school or to time the births of their children to influence school starting age.

The results also speak to the validity of starting age (or quarter of birth) as an instrument for educational attainment as used by Angrist and Krueger (1991). In Norway, individuals are required to stay in school for a particular number of years of school, not until a certain age. As a result, starting school at a different time will have no effect on educational attainment through compulsory schooling legislation. Since we do not find longer-run effects of school starting age on earnings (once we consider a sample aged at least 30), school starting age or quarter of birth may be a valid instrument for educational attainment.

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Table 1: Means of Selected Variables
(Standard Deviations in Parentheses)

	Men	Women
School Starting Age (SSA)	7.3 (.3)	7.3 (.3)
Expected School Starting Age (ESSA)	7.2 (.3)	7.25 (.3)
Completed Education	12.4 (2.4)	12.8 (2.5)
IQ Test Score	5.1 (1.8)	
Teenage Birth		.08 (.28)
Birth Within 12 years of ESSA		.06 (.23)
Log Earnings at age 24	11.37 (1.03)	11.07 (1.09)
Log Earnings at age 35	12.57 (.74)	12.08 (.89)
Full-time Employee	.70 (.46)	.48 (.50)

Table 2: First Stage Effect of Expected School Starting Age on School Starting Age (Standard Errors in Parentheses)

	Men			Women		
	2SLS	2SLS Discontinuity Sample	2SLS with Fixed Effects	2SLS	2SLS Discontinuity Sample	2SLS with Fixed Effects
	(1)	(2)	(3)	(4)	(5)	(6)
Expected School	.804**	.736**	.806**	.820**	.751**	.817**
Starting Age	(.013)	(.014)	(.002)	(.010)	(.013)	(.002)
Observations	739261	117458	383323	702329	111924	350927
R-squared	.56	.52	.56	.60	.53	.61

+ significant at 10%; * significant at 5%; ** significant at 1%

Sample includes cohorts born between July 1962 and June 1988. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in columns (1), (3), (4), and (6) also include a linear trend that runs from July to the following June. In addition, columns (2) and (4) contain family fixed effects and birth order dummies.
The Discontinuity Sample includes only persons born in December or January.
The sample used with family fixed effects includes only families in which there are at least 2 boys or 2 girls.
Standard errors in columns (1), (2), (4), and (5) allow for clustering by birth cohort (defined in months).

Table 3: Effect of School Starting Age on IQ Test Scores of Men (Standard Errors in Parentheses)

	OLS	2SLS	2SLS Discontinuity Sample	2SLS with Fixed Effects	2SLS <11 Years of Education	2SLS 12 or more Years of Education
	(1)	(2)	(3)	(4)	(5)	(6)
School Starting Age	-.754	-.060	-.042	-.066	.003	-.087
	(.027)	(.013)	(.017)	(.017)	(.030)	(.015)
Age at Test	.006	.224	.223	.202	.174	.219
	(.014)	(.012)	(.026)	(.014)	(.020)	(.012)
Observations	664012	664012	105850	321745	89162	354346
R-squared	.0172	.0020	.0031	.0136	.0113	.0020

Sample includes cohorts born between July 1962 and June 1988. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in columns (1), (3), (4), (5) and (6) also include a linear trend that runs from July to the following June. In addition, column (4) contains family fixed effects and birth order dummies.
The Discontinuity Sample includes only persons born in December or January.
The sample used with family fixed effects includes only families in which there are at least 2 boys.
Tests are administered to boys at approximately age 18.
Standard errors in columns (1), (2), (3), (5), and (6) allow for clustering by birth cohort (defined in months).

Table 4: Effect of School Starting Age on Educational Attainment
(Standard Errors in Parentheses)

	OLS (1)	2SLS (2)	2SLS Discontinuity Sample (3)	2SLS with Fixed Effects (4)
Men				
School Starting Age	-.756 (.028)	.009 (.022)	.024 (.023)	-.062 (.028)
N	514662	514662	81918	246793
Women				
School Starting Age	-.741 (.027)	.038 (.023)	.072 (.027)	.054 (.029)
N	489794	489794	78502	226096

Sample includes cohorts born between July 1962 and June 1979. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in columns (1), (2), and (4) also include a linear trend that runs from July to the following June. In addition, column (4) contains family fixed effects and birth order dummies.
The Discontinuity Sample includes only persons born in December or January.
The sample used with family fixed effects includes only families in which there are at least 2 boys or 2 girls.
Standard errors in columns (1), (2), and (3) allow for clustering by birth cohort (defined in months).

Table 5: Effect of School Starting Age on Teenage Childbearing
(Standard Errors in Parentheses)

	OLS (1)	2SLS (2)	2SLS Discontinuity Sample (3)	2SLS with Fixed Effects (4)
Teenage Birth				
School Starting Age	.003	-.018	-.018	-.008
	(.002)	(.003)	(.004)	(.008)
N	218674	218674	35264	70279
Birth within First 12 Years of School				
School Starting Age	.019	.039	.045	.051
	(.002)	(.003)	(.003)	(.007)
N	218674	218674	35264	70279

Sample includes women born between July 1962 and June 1969. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in columns (1), (2), and (4) also include a linear trend that runs from July to the following June. In addition, column (4) contains family fixed effects and birth order dummies.
The Discontinuity Sample includes only women born in December or January.
The sample used with family fixed effects includes only families in which there are at least 2 girls.
Standard errors in columns (1), (2), and (3) allow for clustering by birth cohort (defined in months).

Table 6
Effect of School Starting Age on Earnings
All Men
(Standard Errors in Parentheses)

Age:	24	25	26	27	28	29	30	31	32	33	34	35
OLS	-0.055	-0.086	-0.123	-0.145	-0.164	-0.172	-0.180	-0.190	-0.192	-0.194	-0.200	-0.210
	(0.010)	(0.009)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)	(0.012)
	247195	246285	245655	245463	245530	245590	245436	245021	244736	244160	243813	243301
2SLS	-0.092	-0.099	-0.096	-0.065	-0.039	-0.023	-0.010	-0.006	-0.003	0.006	0.003	0.006
	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.007)
	247195	246285	245655	245463	245530	245590	245436	245021	244736	244160	243813	243301
2SLS	-0.138	-0.124	-0.113	-0.079	-0.047	-0.022	-0.020	-0.007	-0.017	-0.010	0.000	-0.009
Discontinuity	(0.011)	(0.009)	(0.009)	(0.008)	(0.012)	(0.009)	(0.011)	(0.010)	(0.008)	(0.008)	(0.007)	(0.008)
Sample	39407	39203	39151	39133	39125	39185	39126	39034	38937	38847	38825	38705
2SLS Fixed	-0.094	-0.083	-0.071	-0.048	-0.048	-0.023	-0.042	-0.022	-0.007	-0.017	-0.023	-0.024
Effects	(0.025)	(0.025)	(0.024)	(0.023)	(0.022)	(0.021)	(0.020)	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)
	86986	86487	86115	86046	85900	85804	85627	85558	85289	84981	84802	84508

Sample includes cohorts born between July 1962 and June 1970. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in rows (1), (2), and (4) also include a linear trend that runs from July to the following June. In addition, row (4) contains family fixed effects and birth order dummies.
The Discontinuity Sample includes only persons born in December or January.
The sample used with family fixed effects includes only families in which there are at least 2 boys.
Standard errors in rows (1), (2), and (3) allow for clustering by birth cohort (defined in months).

Table 7
Effect of School Starting Age on Earnings
Full-Time Men
(Standard Errors in Parentheses)

Full Time Workers	24	25	26	27	28	29	30	31	32	33	34	35
OLS	-0.056	-0.072	-0.087	-0.088	-0.094	-0.100	-0.100	-0.111	-0.109	-0.109	-0.115	-0.120
	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
	129665	136231	146345	158515	168983	176943	182648	186348	188428	189254	189282	188861
2SLS	-0.032	-0.029	-0.029	-0.030	-0.023	-0.010	-0.004	-0.012	-0.003	-0.004	0.008	-0.004
	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)
	129665	136231	146345	158515	168983	176943	182648	186348	188428	189254	189282	188861
2SLS	-0.059	-0.047	-0.044	-0.037	-0.031	-0.021	-0.015	-0.020	-0.017	-0.021	-0.003	-0.007
Discontinuity	(0.008)	(0.007)	(0.006)	(0.005)	(0.006)	(0.005)	(0.007)	(0.005)	(0.006)	(0.004)	(0.004)	(0.008)
Sample	20746	21731	23345	25351	26913	28145	29128	29557	29830	29957	30051	29877
2SLS Fixed	-0.064	-0.024	-0.022	-0.031	-0.025	-0.004	-0.006	-0.022	-0.019	-0.008	-0.001	-0.020
Effects	(0.020)	(0.018)	(0.016)	(0.015)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)
	29748	32146	35805	40252	44808	48180	50815	52831	53876	54296	54350	53931

Sample includes cohorts born between July 1962 and June 1970. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in rows (1), (2), and (4) also include a linear trend that runs from July to the following June. In addition, row (4) contains family fixed effects and birth order dummies.
The Discontinuity Sample includes only persons born in December or January.
The sample used with family fixed effects includes only families in which there are at least 2 boys.
Standard errors in rows (1), (2), and (3) allow for clustering by birth cohort (defined in months).

Table 8
Effect of School Starting Age on Earnings
All Women
(Standard Errors in Parentheses)

	24	25	26	27	28	29	30	31	32	33	34	35
OLS	-0.135	-0.155	-0.172	-0.173	-0.198	-0.212	-0.209	-0.207	-0.213	-0.211	-0.206	-0.204
	(0.012)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.014)	(0.014)	(0.014)	(0.013)	(0.012)
	223449	221934	220888	220497	220418	220571	220681	220993	221639	222258	223185	224220
2SLS	-0.116	-0.098	-0.078	-0.049	-0.030	-0.011	0.004	0.009	-0.007	-0.022	-0.005	0.001
	(0.014)	(0.013)	(0.012)	(0.014)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)	(0.010)	(0.010)
	223449	221934	220888	220497	220418	220571	220681	220993	221639	222258	223185	224220
2SLS	-0.128	-0.122	-0.089	-0.073	-0.031	-0.004	-0.007	0.025	0.006	-0.022	-0.001	0.008
Discontinuity	(0.019)	(0.011)	(0.011)	(0.015)	(0.013)	(0.014)	(0.012)	(0.012)	(0.014)	(0.013)	(0.009)	(0.010)
Sample	36296	36053	35831	35822	35828	35852	35813	35893	36012	36195	36353	36455
2SLS Fixed	-0.148	-0.089	-0.073	-0.078	-0.066	-0.008	-0.007	-0.012	-0.036	-0.040	-0.035	-0.028
Effects	(0.029)	(0.030)	(0.029)	(0.029)	(0.028)	(0.028)	(0.027)	(0.027)	(0.026)	(0.025)	(0.025)	(0.024)
	72149	71275	70608	70473	70416	70266	70430	70611	71129	71610	72177	72633

Sample includes cohorts born between July 1962 and June 1970. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in rows (1), (2), and (4) also include a linear trend that runs from July to the following June. In addition, row (4) contains family fixed effects and birth order dummies.
The Discontinuity Sample includes only persons born in December or January.
The sample used with family fixed effects includes only families in which there are at least 2 girls.
Standard errors in rows (1), (2), and (3) allow for clustering by birth cohort (defined in months).

Table 9
Effect of School Starting Age on Earnings
Full-Time Women
(Standard Errors in Parentheses)

	24	25	26	27	28	29	30	31	32	33	34	35
OLS	-0.088	-0.091	-0.103	-0.103	-0.101	-0.100	-0.106	-0.104	-0.098	-0.100	-0.100	-0.096
	(0.008)	(0.006)	(0.007)	(0.006)	(0.005)	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)	(0.006)	(0.006)
	94519	97644	102629	106820	109084	110130	110426	110430	110532	110400	111072	112093
2SLS	-0.048	-0.048	-0.042	-0.028	-0.020	-0.005	-0.007	-0.002	0.001	0.004	0.002	0.011
	(0.010)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
	94519	97644	102629	106820	109084	110130	110426	110430	110532	110400	111072	112093
2SLS	-0.066	-0.070	-0.058	-0.025	-0.026	-0.007	-0.019	-0.000	-0.009	0.002	0.006	0.009
Discontinuity	(0.010)	(0.005)	(0.008)	(0.006)	(0.009)	(0.004)	(0.007)	(0.008)	(0.008)	(0.006)	(0.005)	(0.006)
Sample	15330	15795	16508	17195	17618	17926	17838	17878	17828	17814	17890	18068
2SLS Fixed	-0.036	-0.060	-0.036	-0.010	-0.025	0.011	-0.010	-0.016	-0.013	-0.020	-0.002	-0.012
Effects	(0.026)	(0.025)	(0.026)	(0.022)	(0.022)	(0.023)	(0.023)	(0.024)	(0.022)	(0.023)	(0.023)	(0.021)
	15921	16514	17883	18981	19793	19807	19797	19929	20022	19827	20047	20460

Sample includes cohorts born between July 1962 and June 1970. All regressions include indicators for year of birth (defined as running from July to the following June). The regressions in rows (1), (2), and (4) also include a linear trend that runs from July to the following June. In addition, row (4) contains family fixed effects and birth order dummies.
The Discontinuity Sample includes only persons born in December or January.
The sample used with family fixed effects includes only families in which there are at least 2 girls.
Standard errors in rows (1), (2), and (3) allow for clustering by birth cohort (defined in months).

Table 10
Effect of School Starting Age By Quartile and Gender
2SLS (Standard Errors in Parentheses)

	Men			Women		
Dependent Variable:	Lowest 25%	Middle 50%	Top 25%	Lowest 25%	Middle 50%	Top 25%
IQ Test Score	-0.060	-0.070	-0.091			
	(0.025)	(0.020)	(0.016)			
	[164794]	[326150]	[161271]			
Teenage Pregnancy				-0.016	-0.015	-0.022
				(0.005)	(0.004)	(0.008)
				[56308]	[109664]	[52702]
Birth within First 12 Years				0.014	0.037	0.067
of School				(0.003)	(0.003)	(0.008)
				[55175]	[110386]	[53113]
Education	-0.025	0.025	-0.070	0.046	0.051	-0.009
	(0.030)	(0.025)	(0.042)	(0.028)	(0.025)	(0.047)
	[139288]	[254363]	[121011]	[126484]	[245711]	[117599]
Earnings at age 24	-0.034	-0.096	-0.140	-0.071	-0.110	-0.190
All Workers	(0.020)	(0.016)	(0.030)	(0.025)	(0.019)	(0.029)
	[64704]	[126086]	[56405]	[55938]	[113643]	[53868]
Earnings at age 35	-0.041	0.016	0.032	0.001	0.005	-0.010
All Workers	(0.015)	(0.010)	(0.022)	(0.019)	(0.014)	(0.017)
	[63128]	[124068]	[56105]	[56157]	[114687]	[53376]

For all outcome variables except earnings at age 24, the reported quartile is the quartile of the distribution of the predicted value of the outcome variable where mother's education, family size, and birth order variables are used to predict the outcome. For earnings at age 24, the reported quartile is the quartile of the distribution of earnings at age 35.

Appendix Table 1
Pre-School Enrollment: Norway

Children's Age:	0-2 Years	0-6 Years	3-6 Years
1963	0.3	1.9	3.1
1970	0.7	2.8	4.3
1975	2.3	7.1	10.6
1980	5.0	20.9	32.0
1985	6.7	27.5	43.0
1990	10.7	35.9	57.9
1995	21.5	44.3	61.7
1997	27.7	50.7	74.4
1998	27.2	51.6	76.9
2000	25.6	52.2	78.1
2002	27.3	56.8	84.0

Source: Pettersen, 2003

Appendix Table 2
Compliance Rates by Year of Birth

Birth Year	Early	On Time	Late
1962	2.1	91.3	6.6
1963	2.2	91.5	6.3
1964	2.5	91.3	6.2
1965	2.0	92.6	5.4
1966	2.3	92.9	4.8
1967	2.2	93.2	4.6
1968	2.0	93.8	4.2
1969	2.1	94.1	3.8
1970	2.0	94.4	3.6
1971	1.7	95.3	3.0
1972	1.5	95.6	2.9
1973	1.5	96.6	1.9
1974	1.0	97.3	1.7
1975	1.0	97.1	1.9
1976	1.2	96.7	2.1
1977	1.3	96.6	2.1
1978	1.3	96.8	1.9
1979	1.4	96.9	1.7
1980	1.4	97.1	1.5
1981	1.5	97.1	1.4
1982	1.4	97.2	1.4
1983	1.3	97.2	1.5
1984	1.3	97.2	1.5
1985	1.3	97.4	1.3
1986	1.2	97.7	1.1
1987	0	98.7	1.3
1988	.7	98.2	1.1

Each number in the Early column refers to the percentage of children in each birth cohort that started school before the year they turned 7.
Each number in the On Time column refers to the percentage of children in each birth cohort that started school the year they turned 7.
Each number in the Late column refers to the percentage of children in each birth cohort that started school after the year they turned 7.

Appendix Table 3

Compliance Rates by Month of Birth

	Early	On Time	Late
January	.10	.90	0.0
February	.04	.96	.01
March	.02	.97	.01
April	.01	.98	.01
May	.01	.98	.01
June	0.0	.98	.01
July	0.0	.98	.01
August	0.0	.98	.02
September	0.0	.97	.02
October	0.0	.96	.04
November	0.0	.94	.06
December	0.0	.85	.15

Each number in the Early column refers to the percentage of children in each birth month that started school before the year they turned 7.
Each number in the On Time column refers to the percentage of children in each birth month that started school the year they turned 7.
Each number in the Late column refers to the percentage of children in each birth month that started school after the year they turned 7.

Appendix Table 4
Robustness Checks
IV Estimates
Effect of School Starting Age on IQ

	Slope of Linear Trend allowed change in February	Cohort-specific local trend	Quadratic trend	Instrumenting SSA with month of birth dummies	Including quartic function of birth cohort (measured in months)	Including family controls (mother's education, birth order, family size)	Including family controls, discontinuity sample
School Starting Age	-0.062	-0.063	-0.061	-0.063	-0.058	-0.072	-0.055
	(0.014)**	(0.013)**	(0.014)**	(0.013)**	(0.013)**	(0.013)**	(0.015)**
Age at Test	0.225	0.224	0.225	0.225	0.221	0.214	0.219
	(0.012)**	(0.011)**	(0.012)**	(0.012)**	(0.012)**	(0.012)**	(0.026)**
Observations	664012	664012	664012	664012	664012	664012	105850
R-squared	0.002	0.002	0.002	0.002	0.002	0.120	0.121

+ significant at 10%; * significant at 5%; ** significant at 1%

Sample includes cohorts born between July 1962 and June 1988. All regressions include indicators for year of birth (defined as running from July to the following June).
The Discontinuity Sample includes only persons born in December or January.
Standard errors allow for clustering by birth cohort (defined in months).

Figure 1

Effect of School Starting Age on Male Log(Earnings) by Age with Local Linear Trend
All Males

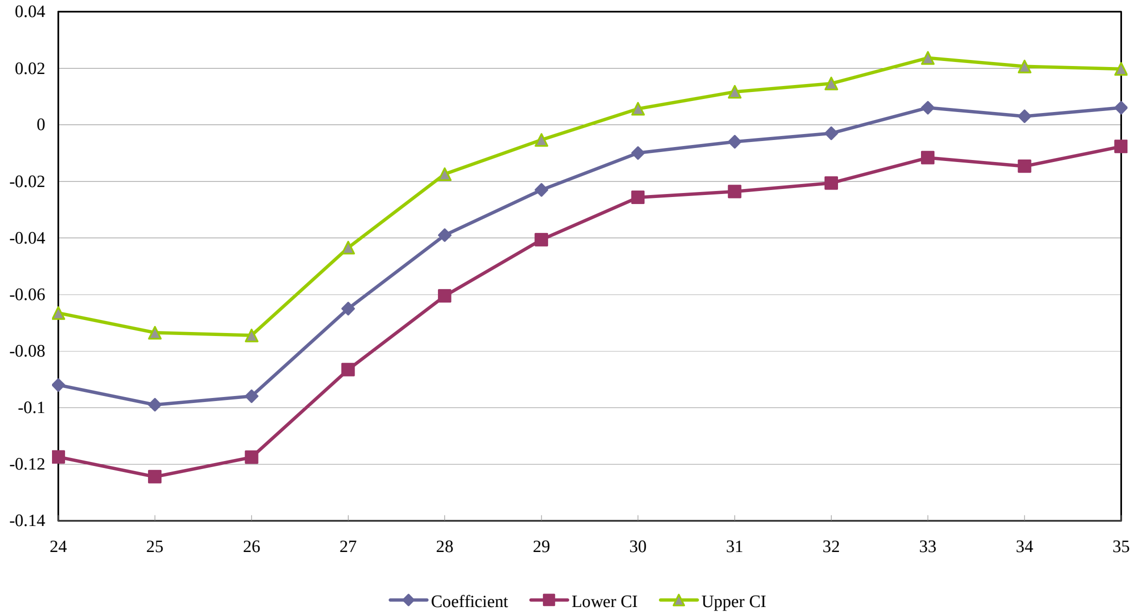


Figure 2

Effect of School Starting Age on Male Log(Earnings) by Age with Local Linear Trend
Full-Time Males

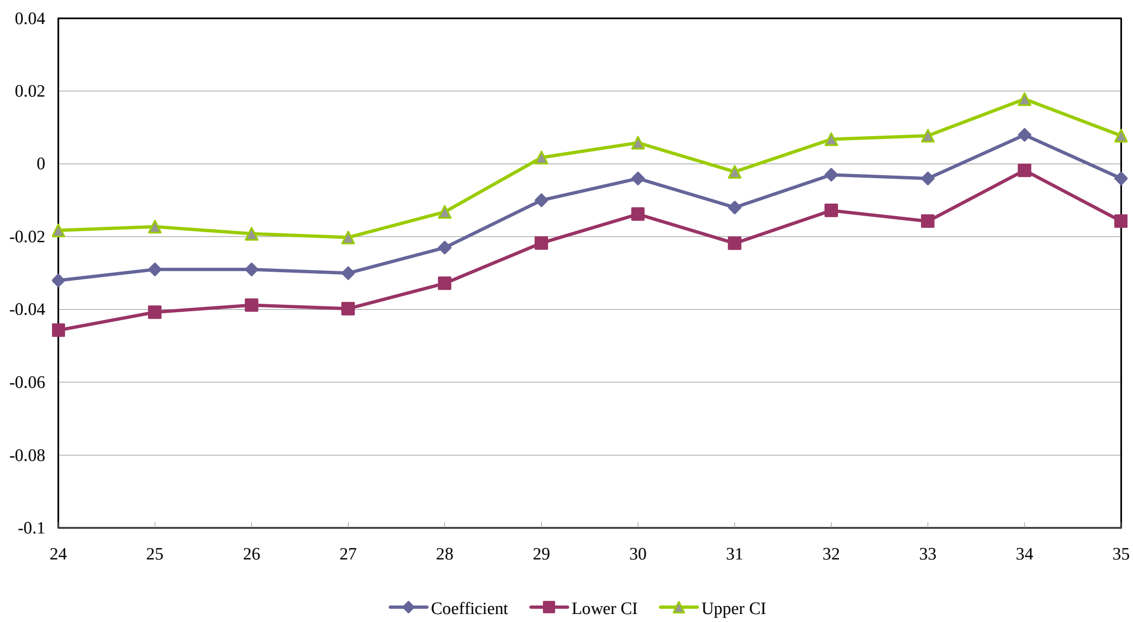


Figure 3

Effect of School Starting Age on Female Log(Earnings) by Age with Local Linear Trend
All Females

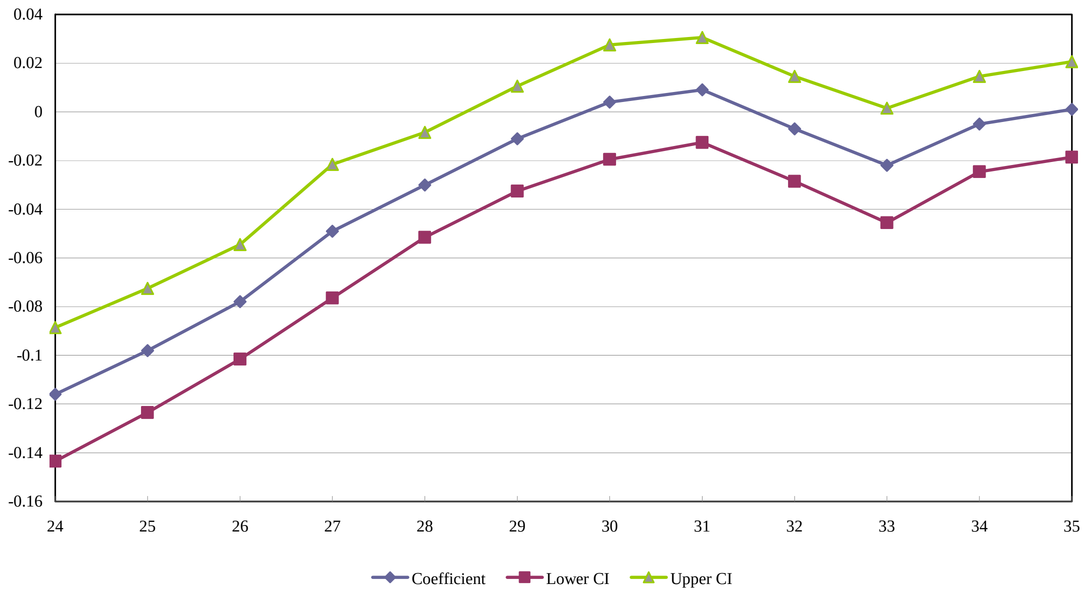


Figure 4

Effect of School Starting Age on Female Log(Earnings) by Age with Local Linear Trend
Full-Time Females

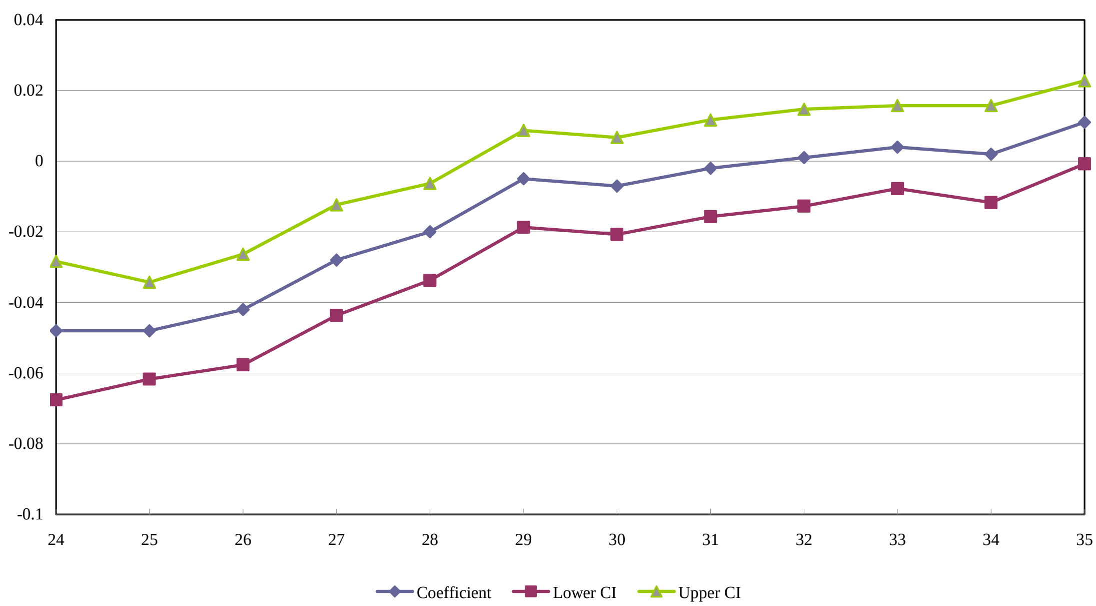


Figure 5

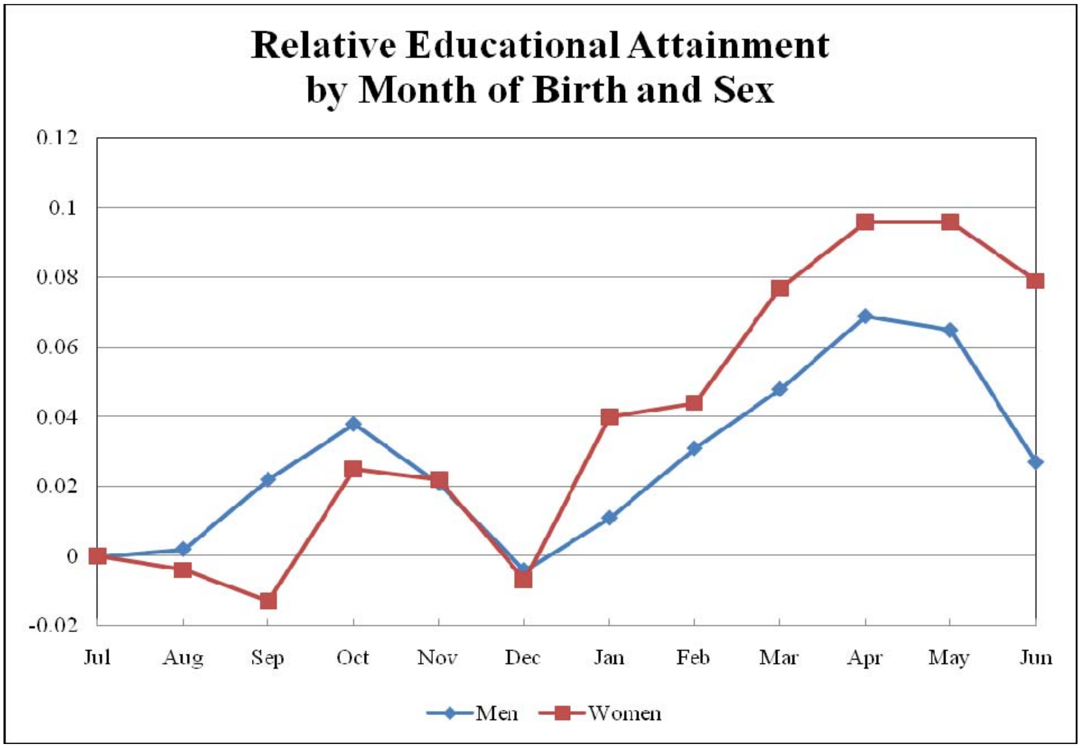


Figure 6

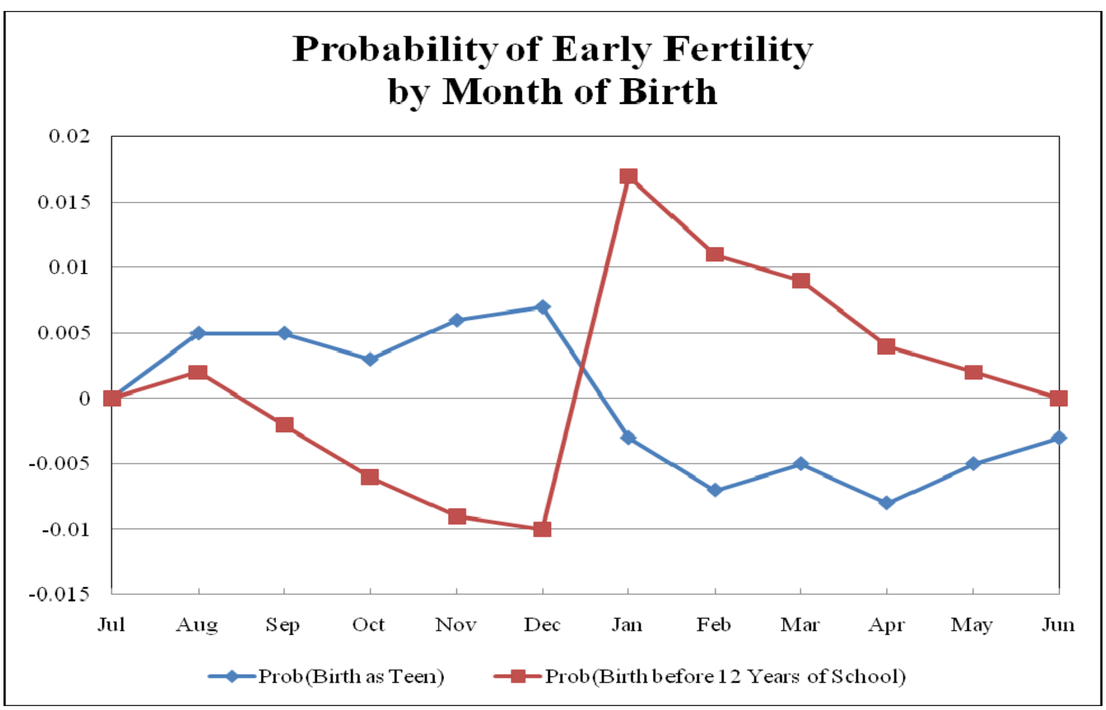


Figure 7

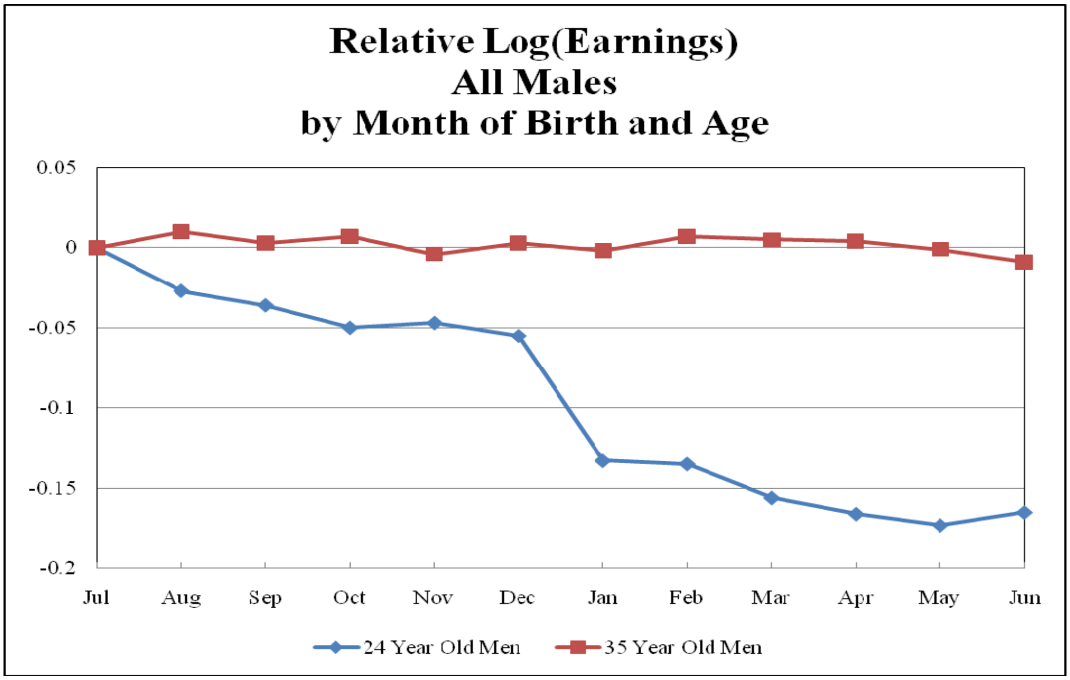


Figure 8

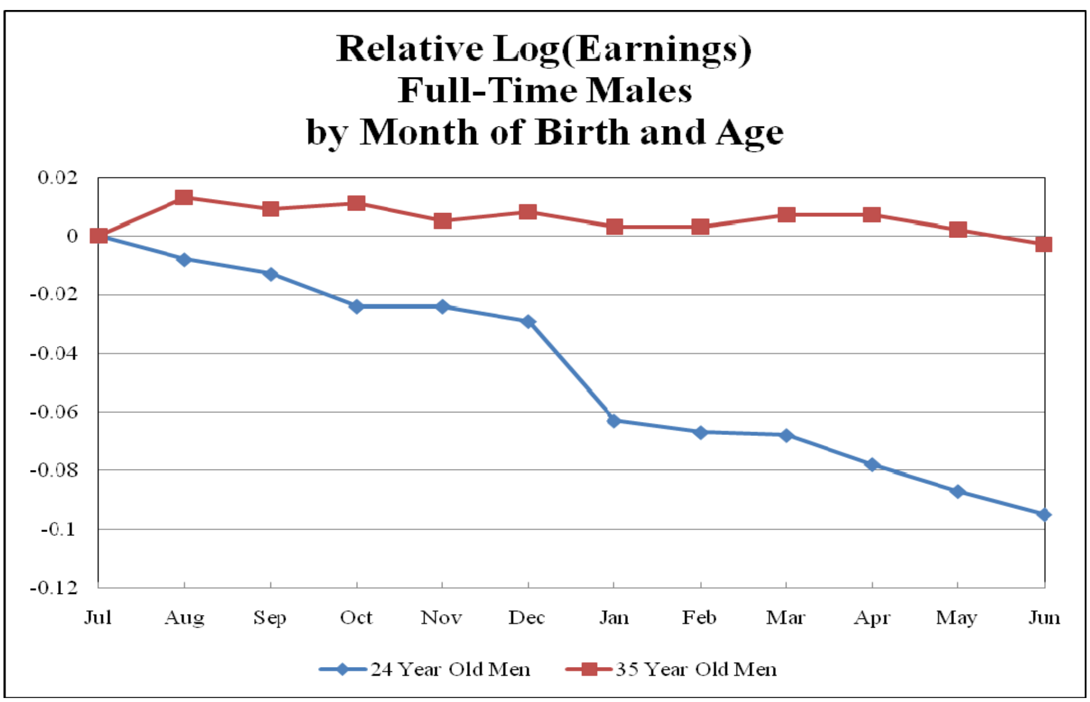


Figure 9

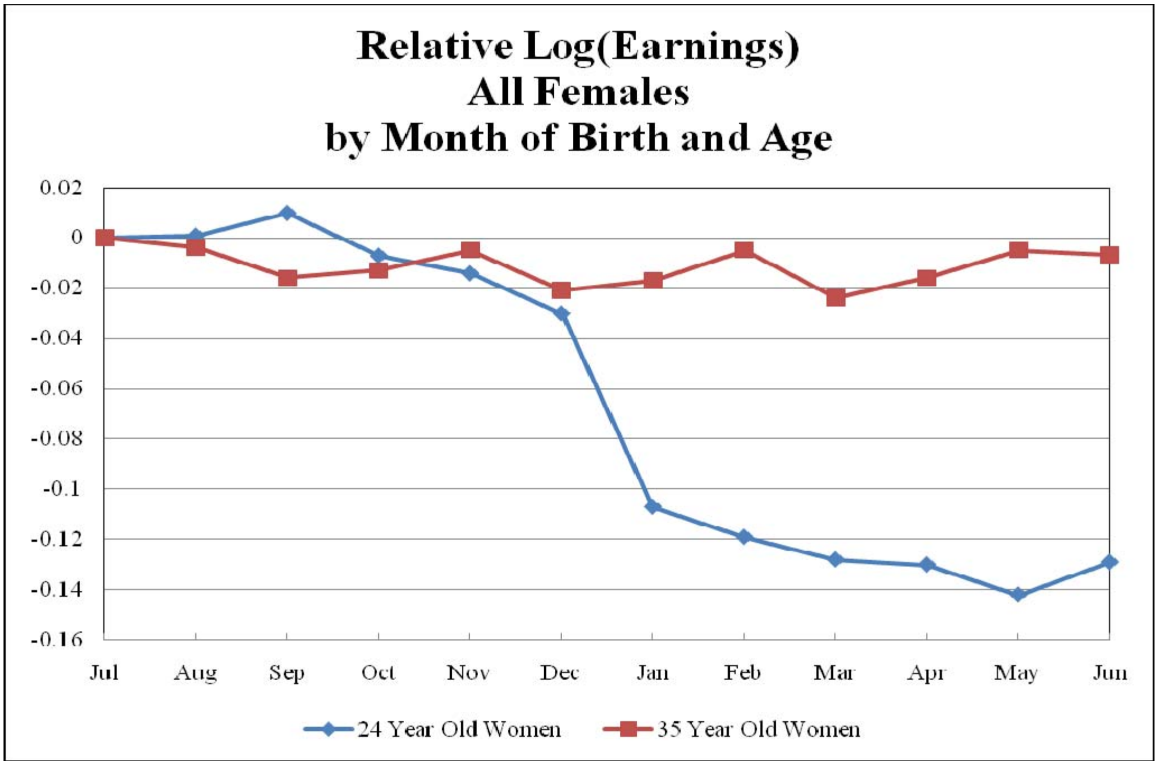
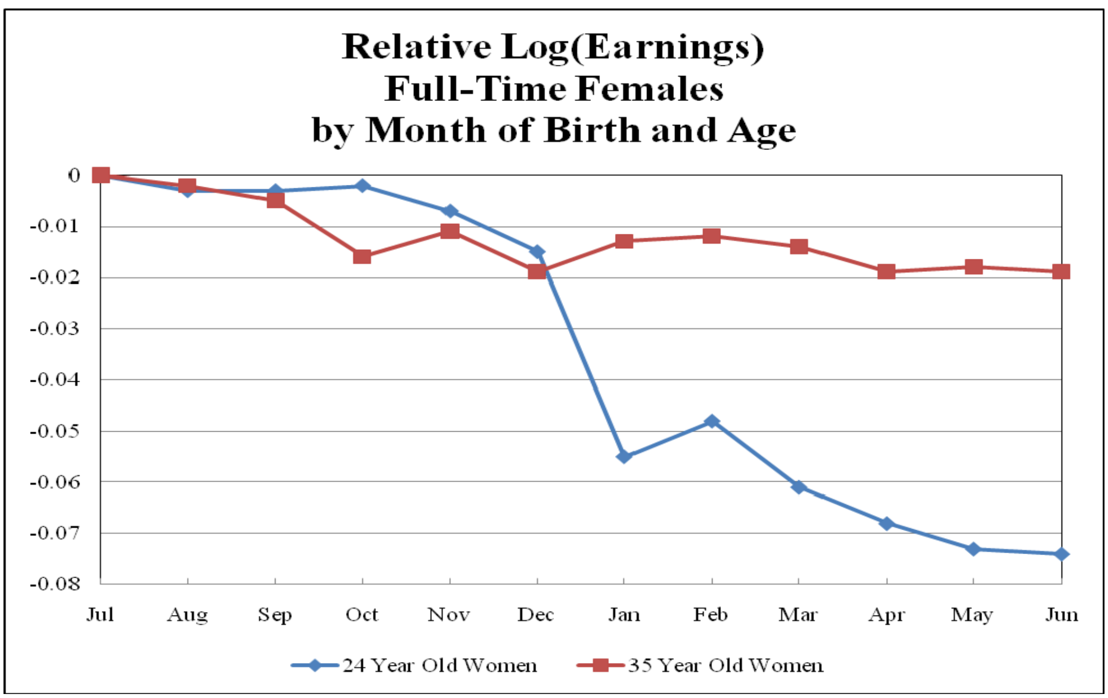
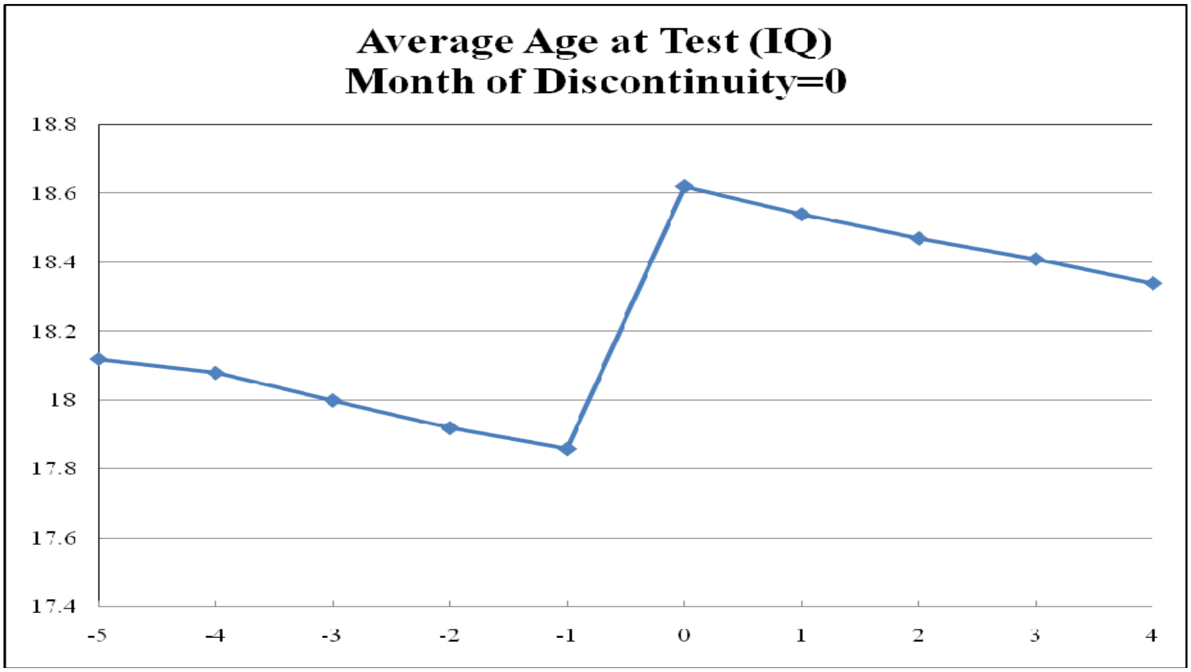


Figure 10



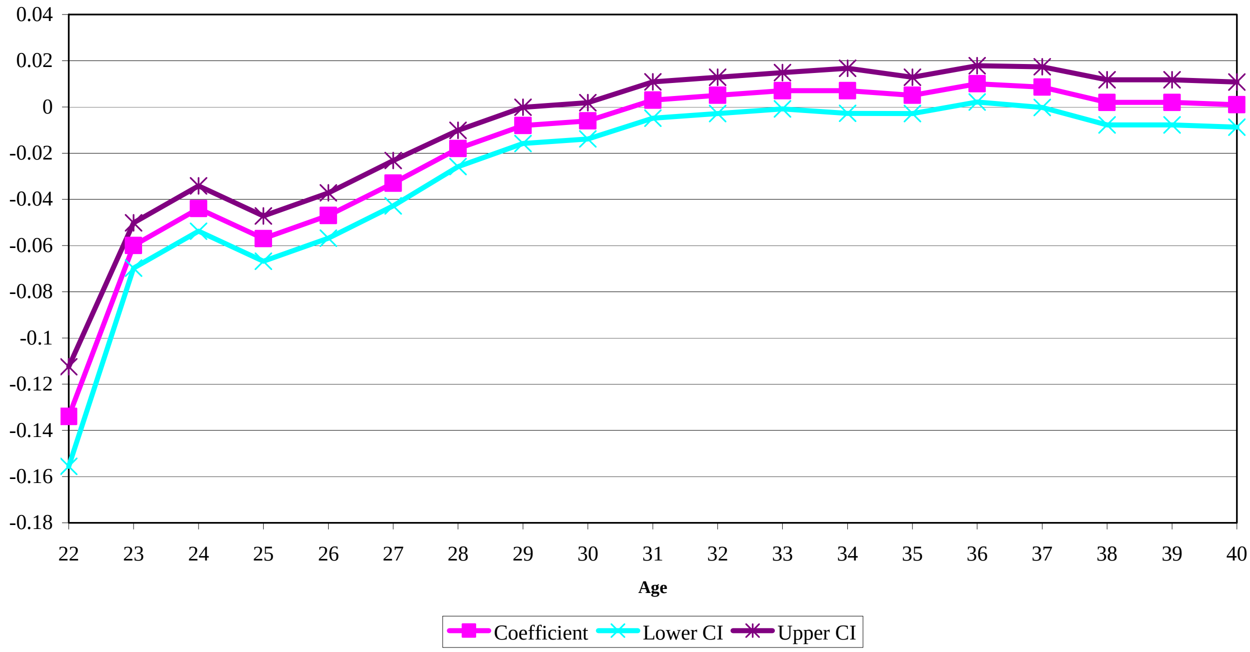
Appendix Figure 1



Appendix Figure 1 illustrates the discontinuity between test month cutoff and age at test in our data. The discontinuity comes from the following: For each cohort of men (though the definition of cohort varies over time), there is a specified year in which they required to take the test. This implies that there are cutoff months such that persons born before that month take the test a year earlier than persons born in that month and after. In the figure, zero corresponds to cutoff birth months and -1 to birth months that are one month previous to a cutoff birth month etc. The figure shows how average age at test varies depending on where the person's birth month is relative to the relevant cutoff birth month for that individual.

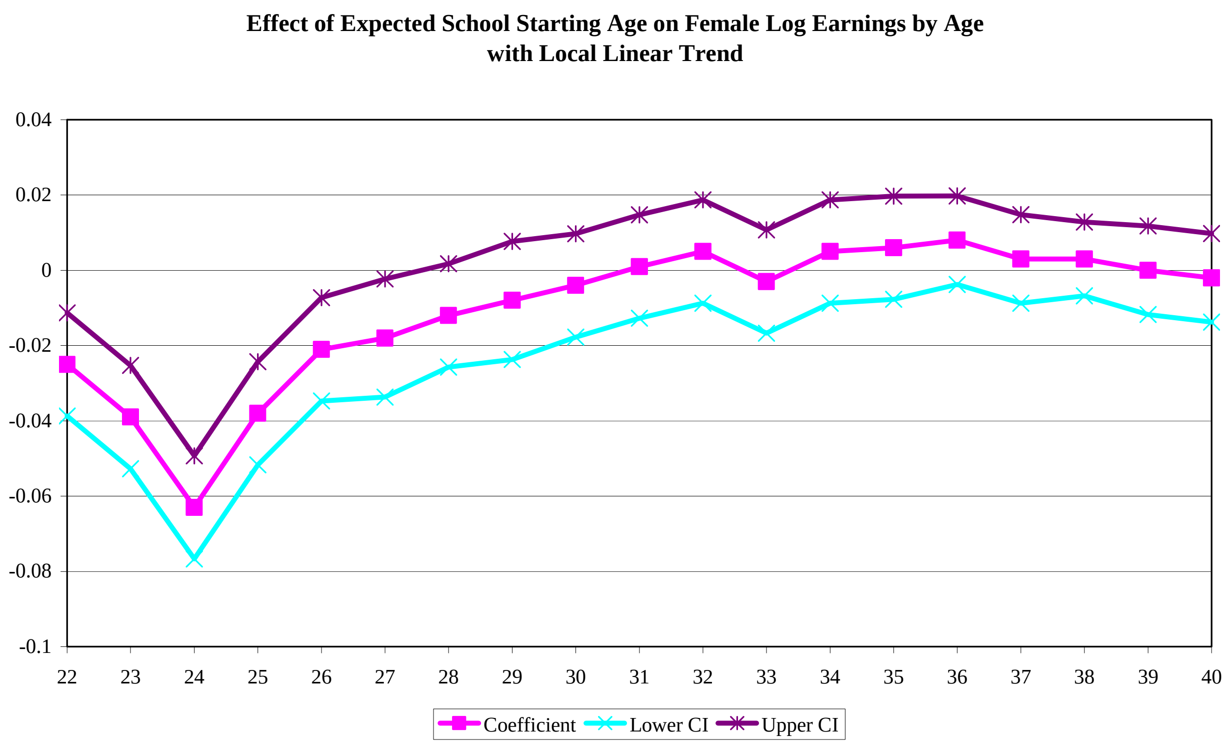
Appendix Figure 2

Effect of Expected School Starting Age on Male Log Earnings by Age with Local Linear Trend



The cohorts used in this figure are 1950-1965.

Appendix Figure 3



The cohorts used in this figure are 1950-1965.